EXPERIMENTAL RESEARCH UPON THE PHENOMENA OF ATTENTION.

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These experiments have been carried on in the Harvard Laboratory with the purpose of discovering the correct interpretation of some curious results first obtained by Prof. Wundt in the study of attention. Prof. James and Prof. Wundt differ in their explanation of the facts and we have attempted to ascertain which, if either, was right. We have come to a conclusion differing from both, which we venture to offer with some assurance that our results will bear the test of verification by other observers. At the outset we shall state briefly the conditions of the experiments as conducted by Wundt and the explanations adduced by him and by Prof. James.

The essential question is whether we can interpret *as simultaneous* two or more disparate simultaneous sensations, and if not, how to explain our errors. For example, as Wundt conducted the experiment, the observer is seated before a graduated dial over which moves a pointer like the minute hand of a clock. The hand does not make continuous revolutions, but vibrates back and forth, each vibration traversing a trifle less than a circumference. A mechanical device enables a bell to be rung at adjustable positions of the pointer, and the subject is required to fix the location of the hand when the sound is heard. The experiment can be complicated, as was done by von Tschisch, through adding electric and tactile stimuli, but his results from these variations show no essentially new factors and are easily explicable under our hypothesis. Furthermore the fundamental conditions are all present in the simpler form, so we shall hereafter deal with that alone. Now, Wundt, found three classes of results. First, *correct results*, when the sound was heard at the position of the pointer where it actually occurred. Second, *positive displacements*, when the sound was heard at a position of the pointer after it had passed the real place. Third, *negative displacements*, when the sound was heard before the pointer had come to the real place. When the

revolutions occurred once in a second he found the errors least frequent. When the rate was faster positive errors predominated, when slower, negative. Wundt has very properly emphasized the peculiarities of different individuals and the differences in the same individual at different times. In this we heartily agree with him and it is no part of our purpose to question any results he has attained. But his explanation strikes us as faulty. He would account for all the variations by the peculiar laws of the ripening of apperception. Thus he assumes that the apperception keeps periodically ripening after each stroke of the bell in anticipation of the next stroke. It may ripen more slowly or more rapidly than the occurrence of the strokes. If faster, then the sound is heard too soon. If slower, it is heard too late. In any event the position of the index at which it is heard is identified as the correct one, and this is obviously determined by the rate of ripening peculiar to the individual. A word should be said at this point in explanation of Wundt's actual procedure in an experiment. He always finds a single revolution of the pointer insufficient to locate the position at which the sound occurs. The motion must continue for a sufficient length of time to allow the sounds to form a regular series. A certain region of the dial is then perceived as the general location, and individual points in this region are then selected until the mind is satisfied.[1]

The general nature of Prof James's criticism will appear from the following quotation from his comments on this subject (Prin. Psych. page 415, Vol. I.): "The bell or other signal gives a momentary sensation, the index a continuous one, of motion. To note any one *position* of the latter is to interrupt this sensation of motion and substitute an entirely different percept—one, namely, of position—for it during a time however brief. This involves a sudden change in the manner of attending to the revolutions of the index; which change *ought* to take place neither sooner nor later than the momentary impression, and *fix* the index as it is then and there visible. Now this is not a case of simply getting two sensations at once and so feeling them—which would be an harmonious act; but of *stopping one* and changing it into another (i. e. exchanging one for another) whilst we simultaneously get a third. Two of these acts are discrepant, and the whole three rather interfere with each other. It becomes hard to 'fix' the index at the very instant that we

[1] For a fuller account of the matter see Physiol. Psych. 2nd ed. II. 264-6, 273-4; 3rd ed. II. 339; Philos. Studien, II. 601 ff. Also Prof. James's Psychol. I. 410 ff.

catch the momentary impression; so we fall into a way of fixing it either at the last possible moment before, or at the first possible moment after, the impression comes. This at least seems to me the more probable state of affairs. If we fix the index before the impression really comes, that means that we perceive it too late. But why do we fix it *before* when the impressions come slow and simple, and *after* when they come rapid and complex? And why under certain conditions is there no displacement at all? The answer which suggests itself is that when there is just enough leisure between the impressions for the attention to adapt itself comfortably both to them and the index (one second in Wundt's experiments), it carries on the two processes at once; when the leisure is excessive, the attention, following its own laws of ripening, and being *ready* to note the index before the other impression comes, notes it *then*, since that is the moment of easiest action, whilst the impression, which comes a moment later, interferes with noting it again; and finally, that when the leisure is insufficient, the momentary impressions, being the more fixed data, are attended to first and the index is fixed a little later on. The noting of the index at too early a moment would be the noting of a real fact, with its analogue in many other rhythmical experiences. . . . . . Wundt's explanation (if I understand it) of the experiments requires us to believe that an observer . . . . shall steadily and without exception get an hallucination of a bell-stroke before the latter occurs, and *not hear the real bell-stroke afterwards*. I doubt whether this is possible, and I can think of no analogue to it in the rest of our experience."

It is no part of our purpose to follow out the differences of opinion merely as such which exist between Professors Wundt and James concerning this matter. We trust the above statement has made their respective positions clear while bringing out the exact nature of the experiment and the points at issue. We believe that there is truth in the explanations of both, but our criticism is that neither at all suffices to exhaust the problem, which proves to be much more complicated than one could possibly suppose from the mere reading of an account. Wundt has justly remarked that the research requires years of experimentation before adequate conclusions can be reached. And yet this is not wholly true. Wundt has worked many years on the subject himself, but his main interest has apparently been to obtain a sufficiently large number of results to justify comprehensive and exhaustive inductions. So far so good. We are, however, unable to see that careful experimentation covering a much shorter period is necessarily imcompetent to isolate with considerable

accuracy the essential psychological processes involved. We think Herr Wundt has overlooked some very important factors and it is on this account that we venture to offer our conclusions. By no means would we on any other ground oppose our experiments, which are only numbered by hundreds, to his, which must mount into thousands.

In front of the dial of our apparatus revolved a pointer, carrying at the back a wire, bent so as to sweep through a movable cup of mercury at the rear of the dial, thus completing an electric circuit which passed through a telegraph sounder, the latter giving a loud click whenever the circuit was closed.

Various reasons led us to abandon Wundt's machine. The specimen at our disposal was faultily constructed in several particulars and accurate experimentation with it seemed almost impossible. In addition to this, the noise made by the clock work, which was the motor power, was very distracting to the observer and we felt sure it must vitiate the results. The principle of the pendulum seems to us unfortunate in this experiment, because the pointer starts from perfect quiet, moves rapidly faster and then slows up to quiet again, thus introducing a constantly changing rate of motion. Most objectionable of all was the bell. The vibrations of course last some time, and the sound of the actual stroke becomes so blended with this after-tone that the difficulties of identifying the stroke with any one position of the pointer are vastly increased. An attempt was therefore made to construct a machine which should possess the following qualifications: first, lack of distracting noises; second, uniformity and steadiness of motion in the rotating pointer; third, an incoming sound which should be short and flat.

The motor power which appeared most convenient and reliable was an ordinary kymograph. To be sure, this is not a noiseless instrument, owing to the whir of the governor, but the noise is by no means of the nature of a disturbing click and we have had no complaint of disturbance from this source on the part of any of our subjects. Of course the ideal motor would be noiseless. The motion was transmitted from the drum of the kymograph to the pointer-shaft by means of a belt of ¾-inch webbing. As an upright to support the pointer a Bradley color-wheel was utilized, the pointer being set on the shaft in the place usually occupied by the color disc. The pointer, made of thin, light wood, gouged on the back to reduce the weight as much as possible, was 63 cm. in length, the tapering end having a length of 47 cm. from the center of the shaft. Pieces of sheet-copper were fastened to the upper end to secure an accurate balance and tests were made occasionally during the observations to ascertain if the equilibrium was perfect.

A dial was made of heavy pasteboard held upright by small wooden supports at each end, which were sunk into a strip of wood; as shown in

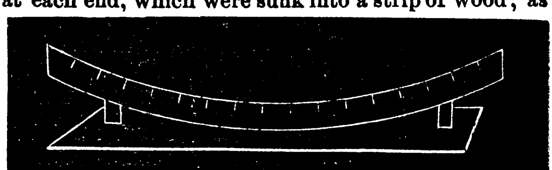


FIG. 5.

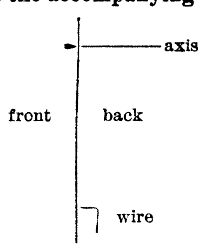
the accompanying figure. The graduation of the dial and the numbering should be as uniform as possible in order that no one portion may

present any greater interest than another. The slightest thing is sufficient to divert attention and so vitiate results.

As the figure shows, the dial was only a portion of a circumference. The inner edge of the dial was 43 cm. from the center, so the *entire* inner circumference would have been 270.18 cm. in length. The available portion of the scale was 47 cm. long—a little more than $\frac{1}{6}$ the entire circumference. This, however, was deemed long enough, as it gave considerable range to vision without requiring any rotation of the head and much less of the eye-balls than would have been necessary with a larger portion of the circumference.

To secure the short, flat sound so desirable we employed an electric telegraph sounder. At the back of the dial and separated from it by the width of its wooden supports was fastened a brass rod of about 3 mm. diameter and of the same curvature as the dial itself. On this ran a carriage bearing a cup of mercury. Down the back of the pointer from the shaft on which it rotated ran a wire which, sweeping through the mercury, completed the circuit passing through the key. The carriage was constructed as follows: upon a small wooden block through which the curved brass rod passed was fastened a small wire-coupler or binding screw. To the head of the screw a bit of glass tubing 6 mm. bore was secured by means of sealing wax. This tube contained the mercury. A small brass wire from the metallic part of the carriage was wound several times around the rod on which the carriage traveled, thus making an electric connection between the rod and mercury. The tension of the wood upon the rod was sufficient to hold the carriage steadily in whatever position it was put, and at the same time not enough to prevent an easy movement from place to place. The whole of the metallic portion of the carriage could be rotated slightly in a plane parallel to the dial and the mercury cup could be raised or lowered by the screw on which it was fastened, thus rendering any necessary adjustment easy and rapid. The carriage, with all that pertained to it, was wholly concealed from the view of the observer, whose eyes were on a level with the dial.

The light brass wire running down the back of the pointer was bent near the top of the latter, as the accompanying diagram shows.



So that while the pointer revolved in front of the dial-scale the end of the wire swept through the meniscus of the mercury behind the dial. A Leclanché battery gave entire satisfaction throughout. The circuit was as follows: From one pole of the battery through the curved brass rod to the mercury; thence, when the pointer completed the circuit by sweeping the mercury, up the pointer to the metal shaft, on which rested a platinum wire connected with the key, and so back to the other pole of the battery. Whenever the circuit was closed the key gave a sharp click.

The dial was divided into spaces of about a centimeter in length, numbered from left to right. The outer gauge of the kymograph and the internal changes of gearing made a large number of rates possible, but

we selected certain ones for the sake of uniformity to represent slow, medium and rapid movement. The time of rotation at these various rates was carefully ascertained by means of a metronome with the following results:

Slow rate—One revolution in 6.06 seconds. Outer gauge at 15. Lower wheel of kymograph in gear, with upper movable wheel in its middle position.
Medium rate—One revolution in 3.6 seconds. Same gearing as above with gauge at 30.
Fast rate—One revolution in 1.2 seconds. Lower wheel out of gear, and upper wheel at extreme left, i. e., toward inner wall of case. Gauge at 30.

The time occupied by the pointer in crossing a single space was accordingly .0219 sec., .0152 sec., .0043 sec., respectively for the different rates.

With these preliminary considerations we are now ready to consider our method of procedure and results. We may say at once that we meet the same facts as Wundt, i. e., correct results, positive errors and negative errors, though the ratio existing between the two sorts of errors does not correspond, nor yet the conditions under which they are found appearing. This, however, does not alter the fact that we have identical cases to explain. A word or two describing the modus operandi of a typical experiment will serve to clear up future explanation. Our usage has been this—to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of observation for the next revolution when any seemingly needed correction is made; to the right, if the sound seems to occur after the pointer has passed the given point; to the left, if before. But now in this very process of correction enters in a *double* process which neither of us observed at the outset, though it was later seen to play a great part. Fortunately it subjects itself with some readiness to independent experiment. What seems at first a bare attempt to get a coincidence of the sound with a certain position of the pointer proves on more acute observation to be a vibratory process in which attention is *primarily* directed in alteration now to the visual factor and now to the auditory. Thus our natural method seemed to be to pick out an approximate point on the dial and then watch for its obliteration by the revolving pointer. Immediately it was obliterated, the attention was turned to the sound to determine whether it had occurred or not. By repeating this process and making needed corrections, a point was attained on the dial which seemed to minimize the interval between the sound and the sight of the pointer obliterating the spot. We say

"minimize" advisedly, because we early discovered how easily one can be deceived into believing a point quite remote from the correct one to be the right one. We could not discover that there was any greater feeling of surety when a correct result was obtained than when an erroneous one. The two sensations *seem* to be essentially simultaneous and yet there is seldom any feeling of security. But in the method just described the obvious attempt was to make the visual element fundamental and then to hitch on the auditory. We were trying to attend to the sound *after* we had gotten a certain sensation of sight. Further introspection showed that however genuine was our attempt thus to exalt the visual factor, the truth really was that attention vibrated and sometimes the sight, sometimes the sound, was made fundamental. If we may be allowed a homely metaphor, now one element, now the other, served as hitching post. Before going on to discuss the varying results we may mention a fact which seemed to modify the above method in a measure. One of us noticed a strong sense of rhythm from the sounds. This was observed expressing itself in more or less unconscious muscular contractions and movements, such as the nodding of the head, the beating of the fingers, etc. This naturally suggests Wundt's ripening of apperception. What part this plays in the results will be remarked later on. Though we lay no stress on this portion of our work, it may be fair to state that the readings which we have obtained from first revolutions show almost without exception positive errors. The first twenty-five final readings of each new subject generally show a predominance of negative errors and then the positive errors come rapidly to the front. So that differing from Wundt we find a preponderance of positive errors. Furthermore we have been unable to detect any constant influence upon the character of the errors due to alterations of speed. We have made this test on the Wundt machine as well as our own, using the extremes of speed.

Now let us turn our attention to the explanation of positive errors in which the sound is identified with a position of the pointer beyond the place where the sound has occurred. This is the error which common sense would assume as natural. In the cases where we obtain a reading from the first revolution of the pointer, the positive error is easily explained as a dragging of the eyes by the moving pointer. It amounts here to a simple reactive experiment. The positive error is explained by the time consumed in transforming the incoming auditory sensation into a motor act, which is in this case the stopping of the eyes. When the eyes have actually stopped, time has therefore elapsed and the pointer having passed on

is thus read too far to the right. The positive error made after deliberation and also after long practice requires further explanation. If we represent the auditory and visual elements by their initial letters, we may express the vibrations which we find occurring in this way—either V—A—V or simply A—V. In the first case attention is directed to the dial, watching for the obliteration of the selected spot by the pointer. Suddenly it is shifted to catch the sound, and this occurring at once, or a moment later, the attention is shifted back again to the pointer, which is now naturally a trifle beyond the actual point of connection. In the second case, A—V, the first step in the above process is omitted. Indeed, it may be disregarded in any event, as the last vibration is the one affecting the result and giving a positive error. When the great ease with which attention fatigues is remembered, the basis of this explanation will be seen to have great strength, and actual experiment will add to its power. To prove the matter conclusively we made a number of experiments in which the sound was purposely made the primary element, to which we then tried to attach the sight of a given position of the pointer, that is, a position selected by the experimenter as approximate. All the errors under these conditions were positive and many of them very large.[1] Such an arrangement makes the experiment essentially one of reaction, though an apperceptive reaction, and the positive error is amply provided for under the necessity of a certain lapse of time requisite for the sound to be registered in the brain and then identified as simultaneous with a given position of the pointer. It may be objected that this is an essential alteration of the real experiment. To this we in a manner assent, simply insisting that the experiment is of such nature as to assume of itself just this (among others) unexpected form. What we have done is by no means to arrange artificial variations, but simply to observe the metamorphoses undergone by the experiment itself. It was not without reason that we remarked at the outset the exceeding complexity of the problem. Occasional positive errors, like occasional

[1] Confirmatory of this are the results of von Tschisch, who found the errors becoming positive when other interrupting sensations—as tactile and electric—were added to sound. The number and disparate quality of the interrupting sensations possessed strong interest, and the form of vibration, represented as A—V, occurred only after the attention had dwelt for a time on the combined sensations here grouped under A. Furthermore, the greater inherent interest of this complex stimulation, as compared with the dial and pointer, would tend to *fix* the attention in anticipation of these combined sensations and so we should obtain, as von Tschisch did, a preponderance of results in which the A—V vibration occurs, thus producing positive errors.

negative errors, are doubtless to be explained as due to the arbitrary and inexplicable fascination of the attention by some one spot on the dial which the observer then tends strongly to regard as the correct one. These freaks of attention, based on the unaccountable interest felt in infinitesimal differences of objects, must be assumed as constant. The positive errors, then, attained from readings of first revolutions are explicable on the basis of time consumed in mere reaction. Those occurring in later readings are also due to the lapse of time involved in an apperceptive reaction. In short, attention vibrates, and this vibration may be of sufficient strength to overpower the factors at work in negative errors and so by the time consumed in the process produce positive errors.

We must next consider negative errors. It will be recalled that we mentioned the fact that in our experiments negative errors predominated during the first twenty-five results, and from that time on positive errors constituted a large majority. In one case the first twenty-five results were all negative errors, while in the ensuing hundred and twenty-five only three negative errors occurred. We may state at once that we consider the factor ordinarily most efficacious in producing negative results to be unconscious, or involuntary, over-correction, which we shall proceed to elucidate. Experience teaches mankind that in the case of moving objects, similar to the pointer, the attempt to stop the eyes from following has generally resulted in their being carried along a little beyond the point desired, and we have become accustomed to rectifying this mistake. What was once a conscious effort has now through psychic education become practically automatic. Instances of education of this general type are common enough. Learning to shoot on the wing, catching or batting a ball are excellent illustrations. But it is still competent for some one to object that this does not explain enough. It may explain why, after the eye has left the pointer, it should swing back to the left, but not why there should even then necessarily be a negative error. Why does not the eye stop at the right of the correct point and so give a positive error? To this we reply that the special rate of motion manifested by the pointer introduces an essentially new psychic, as well as physiological, factor. What we mean is that *rates* of motion as we experience them in life are so exceedingly various as to render accurate acquaintance with any of the faster forms, such as we deal with in this experiment, quite improbable. Thus the subject, when first experimented upon, is really encountering a unique set of experiences to which he has not yet learned to accommodate himself. We think that what happens physiologically (and so explaining

the negative error) is this: there occurs a too complete relaxation of the ocular muscles involved in making the eye follow the pointer, coupled with a spasmodic—and thus too severe—contraction of the reversing muscles. It is in truth an illustration of the difficulties encountered in the accommodation of an organism to a new environment. The case is perfectly analogous to the attempt to catch a falling goblet or vase, where one almost invariably spreads disaster through the spasmodic nature of his effort. Again it may be objected that while our explanation so far may hold good for the cases of first revolutions when the eyes are obviously following the pointer, it is considered fallacious as accounting for negative errors in later cases conducted as we have described, i. e., by watching for the obliteration of a given point by the pointer. The objection has only slight importance, because, no matter how intently the gaze be fixed on the dial, it is practically impossible to prevent a slight vibratory movement of the eyes when the pointer passes, and the spasmodic motion is only reduced in power, but by no means eliminated. In any event this is not the whole story about negative errors. We are, however, convinced that this unconscious over-correction plays a very large part in the production of these errors. In terms of our vibration formula this process would be expressed as V—A—V+ unconscious over-correction.

But the negative error may be produced in another way. Suppose, on a dial graduated and numbered like ours from 1 to 12, the space between 5 and 7 had been settled on by the observer as the general region in which the sound occurred. As the pointer comes round and gets to 5, the attention, which has just previously been concentrated on the visual factor, is instantly shifted to the auditory, and if that occurs in quick sequence the mind feels satisfied that the two have occurred simultaneously. Attention is in this case not shifted back to the visual element. The vibration is expressed as V—A. Apperception time doubtless plays a part here again, but while it enters in to cause error, it has of itself no especial preference for either kind of error so far as we know. A variation which we have next to notice may be wrongly charged to apperception, but we repeat, we know of no facts tending to show *with any conclusiveness* that the apperception time of sound is either longer or shorter than that of sight.

On the other hand there is experimental ground for believing that the photo-chemical process in the retina consumes an appreciably longer time than the vibratory process in the cochlea, so that sight is a slower affair than hearing. Ladd gives the ear an advantage of .049 sec. Now at the slowest

rate employed by us the pointer traversed one space in .0219 sec. Therefore the .049 sec., by which the eye is slower than the ear, is equivalent to more than two spaces traversed by the pointer. We are inclined to regard these particular figures as provisional and subject to alteration under more extended experimentation, but it may be accepted as proven that sight requires longer than hearing and enough longer to play no inconsiderable part in this experiment. Obviously the effect of this fact would be to cause negative errors, for, to take an example, when the pointer was *seen* at $6\frac{1}{2}$ (6 and two spaces) it would really be at 7, and if the correction was made at 7 the sound would be identified with the position $6\frac{1}{2}$ rather than 7, though the time consumed in transmitting the auditory sensation would throw the reading slightly forward toward 7. But it is clear that the effect of this condition of things must be overpowered occasionally by the processes mentioned in our discussion of positive errors, otherwise we should continually get negative results. Aside from our experiments, which point strongly to the fact, there is good *a priori* ground for supposing that experience teaches us to make correcting judgments to compensate for these discrepancies among our senses, so that the gradual disappearance of negative errors under practice is what we should expect, Wundt's results to the contrary notwithstanding. It should be understood that this transition to positive from negative errors occurs without the subject being informed of the nature of his errors, indeed without his knowing whether he is making any errors at all. It seems to be a result of finer powers of discrimination and attention resulting merely from practice. It seems highly probable, then, that when a subject has become practised in the experiment, the part played by unconscious correction of the oculo-motor effects and the tardiness of the photo-chemical processes gradually dwindles into insignificance, and negative errors, when they occur, which is rare with us, are rather due to the vibratory process of attention which we have indicated by V—A. Again we subjected our theory to separate experiment by consciously trying to make the visual factor primary and the auditory secondary without a return to the visual. The result substantiated our hypothesis. In a subject whose total results show a ratio of 3 : 1 in favor of positive errors, this procedure altered the ratio to 4 : 3 in favor of the negative errors. The experimentation was not sufficiently extended in this direction to warrant our ascribing any final validity to this test. It served to satisfy us quite conclusively, however, because the actuality of the process supposed showed itself clearly.

We mentioned earlier in the paper the observation of a feeling of rhythm gained from the succession of sounds and expressing itself in various muscular contractions. We have now to consider the effects of this. It will be remembered that Wundt's explanation hinges on the ripening of apperception. The *natural* rate of ripening may be faster, or slower than the rate of occurrence of the sounds, or identical with it. The different results found their explanation in this fact. Prof. James's explanation finds a somewhat similar basis. The rhythmic feeling we mention should have a very similar effect. With our machine, which gives its sound less frequently than Wundt's, we might naturally suppose that the impatience to "let off" the muscular contractions would produce a tendency to negative error. But there is nothing to substantiate any such sweeping assumption. The peculiarities of the individual may cause this to affect the result in either direction, and by most observers the feeling is not noticed at all. In short, it is another factor which we must consider constant in its influence.

What shall be said of the correct results? It would seem to be sufficient reason for assuming these to be accidental that one has no greater feeling of surety when giving a correct reading than when giving an erroneous one. It is by no means impossible that the different factors which enter into the experiment should occasionally so combine as to produce correct results. Similarly we see no reason for supposing that these same factors may not at any time so combine as to produce either kind of error. We admit the inherent charm of such accounts as those of Prof. Wundt and Prof. James, which seem to involve only a single general principle, but we cannot feel that these at all cover the ground. In any case we do not think the present kinds of apparatus at all competent to decide dogmatically whether the mind does ever succeed in detecting exact simultaneity in such cases aside from mere chance. Our machine is much more accurate in this respect than Wundt's, but it is exceedingly difficult to be sure that a given position of the index is *absolutely* and *exactly* simultaneous with the click. For our own part, we do not believe the mind ever does feel two such stimuli as exactly simultaneous in any other sense than that in which it feels rapily successive and disparate stimuli to be simultaneous. Excessively rapid vibration it is capable of and this answers its purpose. But apparently it has no criterion by which to distinguish exact similarity of disparate stimulations from rapid sequence of the same.

But now a word about Wundt's method of conducting the task which he set himself. The problem which he proposed

he states thus: to examine what shall happen "when we receive a *series* of impressions *separated by a distinct interval* into the midst of which a heterogeneous impression is suddenly brought." But in the method adopted does Wundt really accomplish his avowed purpose? Does the moving of an index hand over a scale really give a *series of impressions separated by a distinct interval?* Not only is this not strictly speaking the case even if one succeeds in following the index hand round the dial, but most of all it is not the case when one adopts the method of fixating a point on the scale for trial reference, repeating the process until satisfaction is gained. Wundt apparently followed the former method, but even if the attempt is thus made to follow the pointer constantly, one soon discovers that he is dealing with no series of impressions separated by distinct intervals. Some portions of the scale will be seen with much greater clearness than others. The constantly changing rate of motion in the hand of Wundt's machine, owing to the pendulum principle employed, adds greatly to this defect inherent in the observation of any body moving across a graduated scale. To obviate this difficulty we attempted to construct an apparatus by means of which a *genuine series of impressions separated by distinct intervals* should be attained. We succeeded in producing a device by which the letters of a series were presented to the eye one at a time. The sound was made to coincide with one of the letters. Two forms of the apparatus were employed, the differences between them appearing from the following description:

FIRST FORM.—Nearly the same materials were used as in the machine previously described. The scale and pointer were removed to the rear of the support, the pointer being attached to the other end of the shaft. Both scale and pointer served simply to maintain the old method of electric connection. At the front, in the place formerly occupied by the pointer, was placed a circular piece of paste-board, 40.5 cm. in diameter, at the circumference of which were placed the letters of the alphabet arranged at regular intervals. The letters were from a set of Dennison's alphabets, size 31. The circle bearing the letters was made to revolve as before by means of the kymograph. About six inches in front of the revolving circle was placed a wooden box large enough to admit the head with ease and not too deep to offer inconvenience to the observer. In the back of this was cut a window 1 cm. square and exactly on a level with the position of the lowest letter on the circle. The box was blackened on the inside to prevent any distraction to the eyes. The position of the box and its distance from the circle could of course be adjusted to the observer. The circle being fitted on to the shaft like a color-disc, it was only necessary to rotate it on its axis to secure the exposure of any letter and then fasten
ening the screw which held it in position.

SECOND FORM.—In the form just described the observations were necessarily made with the use of only one eye. The speedy fatigue under these conditions led us to alter the form as follows: In place of the

revolving circle bearing letters to be viewed through a stationary window, we utilized a circle of black paste-board slightly larger than the last and like it revolving on the shaft. Near its edge a small window was cut through which a series of stationary letters behind could be viewed as the window exposed them one by one. We used a smaller set of letters for this, No. 21. They were arranged upon the arc of a circle whose radius was equal to the distance from the center of the shaft to the lower edge of the window in the paste-board circle. They were separated from each other by spaces of about 1 cm. The adjustments could be made as before. To avoid excessive rotation of the head and eyes, we arranged so that only about ½ of a circumference was exposed for experiment and this on a level with the observer's eyes.

These variations in form have had two points of value for our conclusions. In the first place we find that when you have a genuine series of visual factors regularly succeeding one another your results become disturbed by the effects of after-images. The consequence is an increase in negative errors. For example, suppose in our experiments the electric contact was made at the letter C. It frequently happened that the after image of B or A (which had gone before) would come out with sufficient vividness to be identified with the sound and the attention, being then *set* for A or B on the next revolution, would tend to settle on one of them as the correct letter. The occasional vividness of these after images was very marked and we can now see that it may well play some part in the original form of the experiment. The second consideration is this: that conducting the experiment as Wundt proposes with a series of impressions separated by regular intervals into which is introduced a heterogeneous impression, there is no appreciable effect upon the result other than the effect just noticed of allowing after images to play conspicuous part. This is an additional reason in our minds for distrusting any explanation which regards the facts as capable of arrangement under any one general law, such as the ripening of apperception. The factors causing the different results are far too heterogeneous to admit any such arrangement and, so far as we can see, they *may* at *any time* combine in such relation as to produce either kind of result.

In conclusion, allow us to repeat that we by no means regard our results as warranting any very comprehensive inductions. We question the possibility of making such for many years to come. We hope to have done some service in bringing out a few of the peculiarities in the phenomena of attention and to have suggested in a measure the unexhausted richness of a very simple experiment in revealing these. We take great pleasure in acknowledging our obligations to Prof. James and Dr. Herbert Nichols for much valuable advice and suggestion.